Peer-review report of

Pammi, T., Ceccarelli, C., Hibbs, S., Chase, J., Hamid, N. & McEwen, F.S. (2026). Adverse childhood experiences and violent extremism: A scoping review of the literature. *advances.in/psychology*, 1, e422150. https://doi.org/10.56296/aip00065

Contents

1. Round 1
2. Round 2
3. Round 3

Round 1

Editor

Date: 2026-03-06

Dear Authors,

Thank you for submitting your manuscript, “Adverse Childhood Experiences and Violent Extremism: A Scoping Review of the Literature,” for consideration in our special issue, *Advances in the Psychology of Violent Extremism*. We have received feedback from two scholars with core expertise in this topic. We are grateful for their detailed and constructive comments. As is standard for this journal, we both assessed the manuscript independently before reading the evaluations.

We all agree that your manuscript addresses a clear gap in the literature and has the potential to make a substantial impact. For instance, Reviewer 1 notes that “the topic is highly relevant” and that the methodology is “satisfactory and adequate.” Reviewer 2 commends you “for conducting such a relevant review. I also found the text easy to read and the tables are nicely structured and add value to the text.” We fully agree with this positive evaluation.

At the same time, as can be expected when two experts evaluate such a rich paper, both reviewers identified notable issues that require your attention. Our reading converges with their evaluations, and we therefore invite you to submit a revised version of your manuscript. The reviewers devoted considerable effort to providing thoughtful feedback; while we ask that you respond to each of their points, we will not reiterate them in full here. Instead, we wish to highlight overarching issues that we and the reviewers consider critical in a revision. However, the revised manuscript must address all of the points highlighted by both us and the reviewers.

As Reviewer 1 notes, you must exercise greater caution when inferring causality from studies that generally do not allow for such conclusions. On line 282, when detailing mechanisms, please avoid causal terms (e.g., “effect”, “affected”) to describe possible processes based on correlational data. Many studies testing mediation, including those using network analysis, are strictly correlational, meaning numerous pathways are possible. Furthermore, entirely avoid using words like “predicted” when describing these results.

The issue of causality requires significantly more attention in the discussion. Almost none of the included studies allow for causal inference. We also want to highlight that you describe long serial mediations (e.g., line 396) that are entirely speculative, as they rely on correlational network analysis or other standard mediation techniques.

Because most of the empirical evidence does not permit causal conclusions, it would be beneficial to provide some theoretical rationale in the introduction for why adverse childhood experiences may be relevant to violent extremism, even within the context of a scoping review. This addition would help motivate the objectives of the review. While the introduction clearly summarizes the extensive literature linking adverse childhood experiences (ACEs) to a wide range of negative outcomes such as interpersonal violence and criminal behavior, the transition from this literature to violent extremism remains somewhat abrupt. While the manuscript convincingly documents

associations between ACEs and later violence, it would strengthen the motivation for the review if the authors more explicitly articulated why ACEs are theoretically expected to be relevant to pathways into violent extremism specifically. For example, the introduction could briefly discuss emerging empirical findings, case studies, or theoretical frameworks suggesting that early-life trauma and adversity may contribute to processes relevant to radicalization (e.g., grievance formation, aggression, identity threat, or vulnerability to extremist narratives). Reviewer 2 provides a range of frameworks that can help you discuss the potential theoretical link between adverse childhood experiences and violent extremism. You can then use these frameworks to contextualize your results later in the manuscript. This approach will also make your findings less dependent on the speculative models provided by the original authors whose work you review. Providing this conceptual bridge would also help clarify the rationale for extending the ACE–violence literature to the domain of violent extremism and further justify the importance of the present review.

In lines 300 to 327, it is not always clear whether the processes are supported and tested by the authors' data within the respective studies. Please ensure you always clarify how these processes were reflected in the data. If a process is purely theoretical and not backed by data in the same study, it should not be mentioned. Additionally, clarify whether these processes are based on qualitative or quantitative data. Please also limit the corresponding discussion section to studies that actually test these processes. Note again that the data are most likely unsuited for testing the “direction” of associations. Therefore, please also discuss the possibility of reverse causality relative to what the original authors assumed.

Both reviewers request more information regarding your conceptualization of adverse childhood experiences in the introduction, as well as the operationalization used in your search strategy, which is critical for your inclusion criteria. We concur with this assessment. Because we are catering to a broad audience, many readers will not know what the most prominent subforms of adverse childhood experiences entail.

In addition, the manuscript would benefit from a clearer conceptual definition of violent extremism. In the current description of the inclusion criteria, the term appears to encompass a broad range of outcomes, including individuals who were part of extremist groups, those who supported extremist ideologies, and general population samples in which “violent extremist attitudes” were assessed. These categories reflect conceptually distinct phenomena (e.g., actual involvement in extremist organizations, attitudinal support for extremist causes, or survey-based endorsement of political violence). Without a clearer definition, it becomes difficult to determine what the review is actually synthesizing. We therefore recommend that the authors more explicitly define what they mean by violent extremism and clarify how different outcome types (behavioral involvement, intentions, and attitudes) were conceptualized and handled in the review.

More broadly, the description of study characteristics suggests that the concept of violent extremism is operationalized very broadly across the included studies. The review appears to combine a wide range of outcomes, including individuals convicted of terrorism offences, members

of extremist organizations, individuals supportive of extremist ideologies, mass shooters with ideological interests, and general population samples measuring variables such as exposure to extremist content, knowing someone involved in extremism, openness to violent activism, or “violent extremist attitudes.” These indicators likely correspond to distinct stages along the radicalization process (e.g., exposure, attitudes, support, organizational involvement, and violent action). Clarifying how these heterogeneous outcomes were grouped under the umbrella of violent extremism would substantially improve the conceptual clarity of the review.

The manuscript also lists indicators such as searching for extremist content online or knowing someone involved in political extremism as measures used in the included studies. These variables appear to capture exposure to extremist content or networks rather than violent extremism itself. It would therefore be helpful if the authors clarified how such indicators were conceptualized within the review framework and whether they were treated as measures of violent extremism, radical attitudes, or broader risk factors for radicalization. Distinguishing these levels would again improve the conceptual clarity of the review.

Both reviewers suggest ways to improve your methodological approach. They note that your choice of databases may have been too narrow due to a strong focus on psychology, which seems restrictive for a scoping review covering these specific constructs that are studies in many adjacent fields. We agree and request that you widen your search by including more interdisciplinary databases. Please also consider incorporating grey literature into your search, as Reviewer 2 suggests.

Relatedly, both reviewers noted that information about reliability at the different coding stages is missing. Although you report high agreement, the statistical evidence for this needs to be formally reported, preferably for each distinct stage (abstract screening, full text screening, data extraction, etc.). We also concur with Reviewer 1 that your quality assessment tool requires a more detailed description. In the methods section, please provide a description of the MMAT and its subindicators. Furthermore, under the “qualitiy of included studies” section, indicate the most common reasons why studies generally received lower scores. It is also somewhat surprising that qualitative studies generally received higher scores; please elaborate on why this occurred. Finally, it is important to factor in study quality when describing and interpreting findings. Specifically, address how each result should be “weighted” when drawing conclusions given the limitations of the corresponding study design.

In addition to these overarching issues, please address the following specific points in your manuscript:

ABSTRACT

Please add the total sample size (N = 15,788) to the abstract, including some percentage ranges for adverse childhood experiences across the different populations.

METHOD

The PRISMA chart shows that out of 34 studies, 8 were excluded. This should result in 26 reports, rather than 27. Please clarify this discrepancy.

Additionally, please make all data and appendices openly available in a repository such as OSF. In the next revision round, ensure that we are granted access to these materials.

RESULTS

The text currently in lines 232 to 245 and lines 329 to 336 should be moved to the discussion section.

When reporting correlations from the original reports, please report the exact *p* values to three decimal places, or as $p < .001$ when applicable. Present all correlations to two decimal places. Alternatively, as Reviewer 1 notes, if you are simply reporting the exact figures from the original work verbatim, please state this explicitly.

On line 260 and in other places, rather than stating that a variable was “not associated with” another, please state that it was “not statistically significantly associated with.” Absence of evidence is not evidence of absence.

In the results section, treating “parents who were never married” as an indicator of family dysfunction appears unusual. This relationship status is the norm in many countries, where other equally recognized forms of cohabitation exist. If the authors of the original reports grouped these categories together, please discuss this as a limitation. If they did not, please use the more accurate indicator provided in the primary studies.

In the section titled “Association between ACEs and violent extremism,” several reported outcomes appear to capture broader constructs related to aggression or general violence rather than violent extremism specifically (e.g., appetitive aggression, lifetime committed violence, or current aggressive behavior). It would therefore be helpful if the authors clarified how such outcomes were interpreted as indicators of violent extremism and whether distinctions were made between general violence, participation in armed conflict, and ideologically motivated extremist violence.

In addition, references to “former armed group members” are conceptually ambiguous, as armed groups may include militias, insurgent groups, or other actors that are not always categorized as violent extremist organizations.

We also found the distinction between the sections “Association between ACEs and violent extremism” and “Association between ACEs and susceptibility to violent extremism” somewhat unclear. In the second section, most of the studies still report associations between ACEs and outcomes such as openness to illegal and violent political action, violent extremist attitudes, or support for political violence. Conceptually, it is therefore not entirely clear how these outcomes differ from those discussed in the previous section, particularly given that many studies in the violent extremism literature rely on attitudinal or support-based measures when direct behavioral indicators are unavailable.

Moreover, the concept of “susceptibility to violent extremism” is not clearly defined, making it difficult to understand how it is analytically distinguished from violent extremist attitudes or behavioral involvement (e.g., association preceding section). It would therefore be helpful if the authors clarified the conceptual criteria used to separate these two categories of outcomes and explained how “susceptibility” differs from other attitudinal or behavioral indicators of violent extremism. As currently presented, the two are treated as separate entities, each with its own section in the result section, but the distinction between the two sections remains somewhat ambiguous.

The section on mechanisms of action is a valuable addition, as it moves beyond simple associations and highlights potential mediating pathways linking ACEs to violent extremism (e.g., through mental health difficulties, conduct problems, or social exclusion). At the same time, it may be useful for the authors to more explicitly acknowledge an important limitation in interpreting these associations. Adverse childhood experiences are relatively common in the general population. For example, the authors describe a large meta-analytic study at the beginning of the manuscript, which suggests that a substantial proportion of individuals report at least one ACE during childhood, whereas only a very small fraction of individuals engage in violent extremism. This implies that ACEs alone cannot be considered a sufficient explanation for violent extremism and that their effects are likely to be indirect, conditional, and highly context-dependent. Research in the field of terrorism studies has similarly emphasized that factors such as childhood adversity or mental health problems tend to operate through complex and heterogeneous pathways rather than acting as direct predictors of extremist behavior (e.g., Sarma et al., 2020). It would therefore strengthen the interpretation of this section if the authors briefly acknowledged the high base rate of ACEs in the general population and clarified that these factors should be understood as potential vulnerabilities within broader causal constellations rather than as standalone drivers of violent extremism.

Finally, on line 279, it is unclear how “achievement loss” is an adverse childhood experience. Please clarify.

DISCUSSION

In the discussion, the authors state that ACEs were “mostly reported at much higher prevalence than seen in general population samples but were not necessarily higher than rates seen in control samples.” While this is an important point, it is not entirely clear from the results section how this conclusion was derived. The section describing ACE characteristics primarily reports prevalence estimates within extremist samples, but there is limited systematic comparison with population base rates. Although a few studies with comparison groups are mentioned (e.g., Dhumad et al., Stemmler et al., Lankford & Silva), the evidence appears mixed and is not synthesized in a way that clearly supports the broader claim made in the discussion. We therefore would like to request that you whenever possibly explicitly compare reported ACE prevalence to general population estimates (from other studies).

The second paragraph summarizing the “dimensional relationship between ACEs and VE” is somewhat difficult to follow conceptually. It is not entirely clear what is meant by “dimensional relationship” or “VE dimensions,” nor how these constructs were operationalized across the cited studies.

The discussion of “cognitive susceptibility” as a pathway linking ACEs to exposure to extremism is conceptually interesting, but several aspects of the argument would benefit from clarification. First, the term “cognitive susceptibility” is not clearly defined and appears to encompass a broad set of factors (e.g., ACEs, mental health problems), which makes it difficult to understand how it differs analytically from the predictors already discussed earlier in the manuscript. Second, the explanation that the direct association between susceptibility and exposure to extremism is weak because susceptibility factors are common in the general population is not entirely convincing, as common risk factors can still strongly predict rare outcomes if conditional probabilities differ substantially. Third, aligned with previous comments, the paragraph appears to conflate several related but distinct constructs—exposure to extremism, susceptibility to extremist attitudes, and participation in extremist activity—which would benefit from clearer conceptual separation.

The results seem to broadly suggest that adverse childhood experiences may explain both why individuals join violent extremist groups and who becomes particularly violent within them. If you agree with this interpretation, it would be an important point to highlight in both the abstract and the discussion.

Your recommendations are very helpful and should receive greater emphasis. Can you present these recommendations in a structured list and make them significantly more concrete? For instance, specify exactly what definitions for adverse childhood experiences and violent extremism should be used. Additionally, noting that full data were often unavailable in the assessed reports, a valuable recommendation could be for future authors to follow open science principles, such as data sharing and pre-registration.

STYLISTIC

There is one extensively long paragraph in the discussion that should be split into shorter, more readable segments.

In accordance with APA 7 guidelines, please use “women” and “men” instead of “males” and “females.”

Ensure that all Latin statistical symbols are italicized, including *M*, *SD*, *n*, *r*, and *p*. This applies to the main text as well as all tables and figures.

Check that there is appropriate spacing before and after mathematical symbols such as = and <.

In all tables, study citations must be presented in APA 7 format (e.g., Author et al., xxxx). If a study has multiple authors, the et al. abbreviation must be applied correctly.

In one of the tables, the symbol “=<” is unclear and should be corrected.

Make sure that all figure titles, captions, and tables strictly follow APA 7 formatting.

Please provide the figure as lossless SVG file inserted into the word file, which can be exported from most programs, including PowerPoint. Always spell out numbers when they appear at the beginning of a sentence.

In Table 1, the sample descriptions appear to be copied verbatim from the original papers. As this is the case, you must either use quotation marks or paraphrase the descriptions. The same applies to Table 2 under the “ACE measure” section.

Please ensure that all references are meticulously formatted according to APA 7 style. For example, all journal titles must be italicized, and DOIs for each article must be presented in URL format. The formatting of multiple authors is currently inconsistent; first names should be represented by initials followed by a period, and the final author should always be preceded by an ampersand. We noted several typographical errors, such as “Stemmler et el.” and inconsistencies like “Pedersen” versus “Pederson”. You will be able to spot other formatting issues using our free checking tool at https://apa.advances.in. Please upload your manuscript file there to check for possible inconsistencies, correct them, and upload again for validation. Note that the tool is probabilistic and you may disregard any false positives it flags.

In summary, we see clear value in this manuscript but believe its full potential has not yet been realized in its current form. Therefore, we invite you to submit a revised version that addresses the points outlined in this letter. Our goal is to help you maximize the impact of your valuable work.

Please submit your revision by May 20. Should you require additional time or wish to discuss how best to address these comments, do not hesitate to contact us. We look forward to receiving your revised work.

Best regards,

Jonas R. Kunst & Milan Obaidi

Reviewer 1

The paper reports a scoping review of 27 papers on the association between adverse childhood experiences and violent extremism. Although the scope of the review is relatively narrow, the topic

is highly relevant. The methodology is, overall, satisfactory and adequate to the stated objectives of this scoping review. However, there are several points I would like to see improved.

Methods

1. I would like more details on the literature search, namely the additional strategies – citation search and expert advice, which are only mentioned in the Prisma graph. Also, please specify the dates when the search was conducted (this item is on the Prisma checklist, but I am not finding it in the paper).

2. The choice of databases is rather narrow, and I was surprised that no political databases were included. Could the authors explain this choice, not necessarily in the paper?

3. There is very little information about abstract screening, other than that Rayyan was used. Was the screening done by the two authors for different batches of abstracts, or were some records double-screened? If so, how many and what was the agreement? Which model was used in Rayyan? Were all abstracts screened, or did the authors take advantage of the ML ranking and only screened a portion of them? What portion was that? What were the stop rules?

4. Similarly, for data extraction, the agreement between the two coders should be calculated and reported.

5. MMAT was used as an overall score; I would appreciate more information in the text as to which criteria make up this score. There is no need to describe the tool/criteria in detail, but I think the reader should be able to understand roughly what aspects were evaluated without having to check the Appendix. Also, please specify who applied the criteria, and if possible, provide interrater agreement for those as well or include it in the coder agreement (see point 4).

Results

6. In the section *Study characteristics*, I'd ask the authors to specify at least the most represented HIC and LMIC. I understand that this information is in the table, but a more detailed summary in the text would be needed. As a side note for the authors' consideration, the terms HIC and LMIC are only used twice each in the entire paper, and using these abbreviations instead of writing them out is perhaps unnecessary, especially since you already use others that are more needed.

7. Section *ACEs characteristics in extremist samples* – I struggled to understand which studies this section refers to (All of them? Only the qualitative/mixed methods ones? How many) and to which level which statements refer to – across studies or within studies? I would suggest the authors specify it for all paragraphs, for instance (line 165-166):

"Among left and right-wing extremists as well as white supremacists…" - Please specify how many studies these represent.

"the most commonly reported ACEs related to household dysfunction..." – Please specify how many studies in this group mention it.

“followed by experiences of maltreatment” – Please specify how many studies in this group mention it.

8. The categories mentioned in this section (household dysfunction, maltreatment) are not defined earlier – could the authors explain where these come from? Are they data-driven or pre-defined?

9. In sections on *quantitative studies* (line 246 and further), results are reported in an inconsistent manner, sometimes to the 2nd, sometimes 3rd decimal, and the effect size is not always reported. I understand this might be due to the way results are reported in primary studies, but if that’s the case, please state it.

10. “Augsburger and colleagues found a significant positive correlation 248 between experiences of violence during childhood and appetitive aggression ($r=0.19$, $p<0.05$), lifetime committed violence ($r=0.36$, $p<0.001$), and current aggressive behavior ($r=0.51$, $p<0.001$)…” (line 248) – I’m unclear if those outcomes really represent VE. Please comment. Perhaps a more detailed specification of the variables that were categorized as VE (and ACEs) in the Methods is needed.

Discussion

11. The authors have included MMAT assessment, but do not discuss how the quality of the studies could affect the conclusions. I’d like a more explicit evaluation: Were any of the conclusions only supported by low-quality studies? Would the conclusions change if these studies were excluded? This could be done within the limitations section.

12. Line 445 - the authors advocate for prospective studies, but I wonder how feasible these would be in practice. Please comment.

Overall

13. I urge the authors to be more careful with causal language across the paper. The studies summarized, including the one study discussing possible processes, do not really speak to causality, and the authors acknowledge that, but some statements may read as causal (e.g., line 355 – ACEs may increase the risk…)

Please note that I did not have access to the Appendices and am therefore unable to evaluate adherence to Prisma or the MMAT assessment.

Reviewer 2

I thank the editor for giving me the opportunity to review this interesting manuscript. The authors conducted a scoping review of the literature on the association between adverse childhood experiences (ACEs) and violent extremism (VE). A total of 27 studies were analyzed and the synthesis revealed that particularly the ACEs that were linked to household dysfunction are prevalent in individuals associated with extremis groups. The authors also found that both social and mental health characteristics may play a role in increasing the susceptibility of people exposed to ACEs to support VE groups. I believe the review fits the scope of the journal advances.in/psychology, and adds to the literature as no systematic review on this topic was yet

available. Overall, I commend the authors for conducting such a relevant review. I also found the text easy to read and the tables are nicely structured and add value to the text. That said, I also think that the manuscript can be strengthened in several ways, and particularly regarding the methodological approach the authors took in conducting their review. Below, I offer my suggestions for improvement per section of the manuscript. I hope my feedback is useful in strengthening the manuscript.

Abstract

- Line 7: The correct database name is “APA PsycInfo” (also relevant for line 69).

Introduction

- I think the Introduction could benefit from a concise theoretical section that explains why ACEs might be relevant to VE. It seems there are several theoretical frameworks that can account for potential pathways from ACEs to involvement in VE. For instance from a developmental and trauma perspective, early abuse, neglect, and household dysfunction may disrupt attachment, emotion regulation, and identity, which increase vulnerability to (extremist) groups that provide structure, belonging, and an outlet for aggression. General strain theory suggests that cumulative (early-life) strains foster anger and resentment which may result in ideologically justified violence. Social learning and social control theories further imply that exposure to violence and weak bonds to conventional institutions during childhood may normalize violent behavior and lower the threshold for joining extremist groups. Finally, from attachment theory can be derived that ACEs may lead to insecure attachment which fuel later quests for meaning and significance that violent-extremist groups promise to satisfy. Touching on these or other theoretical frameworks provides a conceptual rationale for focusing on ACEs as potential risk factors for VE.

Methods Section

- I am a bit worried that the choice of databases might not cover all relevant articles reporting on ACEs and VE. The field of ACE research is quite interdisciplinary (e.g., psychology, psychiatry, public health, social work, criminology, and education), and I therefore advise the authors to search at least one multidisciplinary database like Scopus or Web of Science. While the APA PsycInfo database is excellent for picking up on studies in (general) psychology, mental health, and behavioral sciences, I also advise to search at least one additional more focused database like Criminal Justice Abstracts (criminology) or Social Services Abstracts or SocINDEX (sociology / social work), provided the authors have access to these databases. The authors do mention that “the search was limited to three databases” in the Discussion (lines 436-437), but I don’t see why only searching narrow-focused databases follows “the scoping nature of this review” (line 436).

- The authors do not report whether they attempted to identify relevant grey literature, but I can imagine there are governmental and NGO reports out there (e.g., from WHO, CDC, UNICEF, or perhaps child protection agencies) that report on ACEs and VE. I advise the authors to also search for relevant grey literature to (try to) reduce the risk of publication bias.

- The criteria for study eligibility can be strengthened by providing details on age (were only studies included that assessed ACEs occurring before the age of 18 following the original CDC-Kaiser ACE study?), and the type of ACEs the authors focused on. For instance, peer/sibling violence and bullying can be regarded as childhood trauma but are often not measured in original ACE scales.

Further, online abuse, institutional abuse (e.g., abuse in residential care / boarding schools / religious institutions), racism and discrimination, and gang violence in the rearing area of a child can be regarded as trauma exposure, but the text is insufficiently clear what ACEs were relevant for inclusion. Similarly regarding VE, I think it is good to explicitly inform readers what types of VE were relevant for inclusion: Politically motivated violence as well as religious, ideological, racist, and gang-related violence? And did the authors distinguish between VE and terrorism in some way?

- Please add references to line 76 when referring to “relevant reviews”.

- It is unclear to me what “implementation outcomes” (pg. 4, line 101) refer to. Please clarify.

Results Section

- Reading the Results section there were two issues that struck me:
(1). The section on associations between ACEs and susceptibility to VE (lines 263-281) clarify that the authors were (also) interested in studies that did not necessarily report on VE itself, but on openness to / susceptibility to / attitudes towards VE. This was not clear to me after reading the Methods section, and I think particularly the section on eligibility criteria can be clarified on this.
(2). The authors state that they conducted a “narrative synthesis” and “identified common themes and variations” following Popay et al. (2006) (Methods section, lines 100-104), but the Results section does not provide information on the different steps or stages of the synthesis. I am not an expert on the stages proposed by Popay and colleagues for conducting a narrative synthesis, but for transparency and reproducibility it would be helpful to report on how the different steps were carried out and how they led to the reported results. Providing these details would strengthen confidence in the synthesis and its conclusions.

- As ACEs differ in nature, it would be informative for readers to add a column to Table 1 that describes the type of ACEs that were studied in each included study.

- The authors report on interrater reliability in the screening and selection process (pg. 4, line 92), but I don’t see information on reliability in the Results section. Can the authors add information on interrater reliability during the extraction phase?

- Lines 188-189: For consistency, please add percentages.

Discussion

- Following my suggestion to incorporate theory into the Introduction (see above), I think the Discussion would benefit from a section on the extent to which the included studies draw on the theoretical frameworks that are available and explain the link or mechanisms between ACEs and VE. Could the authors provide some indication of which frameworks have and have not been explicitly used or tested in the included studies?

- I suggest breaking the rather long paragraph on page 28 into two separate paragraphs.

Round 2

Editor

Date: 2026-05-20

Dear Authors,

Thank you for so thoroughly addressing the issues raised in the previous round of review. Your manuscript was already strong, and it has become even more robust. The revised structure in which you present your results, including your more rigorous criteria for the inclusion of studies, has greatly improved the clarity with which the findings are communicated.

We believe the paper can be accepted pending a set of minor revisions. Please address the small remaining points raised by the two reviewers. Regarding coder reliability for data extraction, we consider your cross-checking procedure to be sufficient, but please briefly acknowledge the absence of a formal reliability check for this step as a limitation in your discussion.

In addition to the reviewers' remaining points, please address the following:

1. Please consolidate all appendix content into a single supplementary information file. This will be published with its own DOI alongside the research itself.
2. There is a missing comma before "(c)" in line 91.
3. In line 141, please spell out numbers when they begin a sentence.
4. In line 264, was the difference between (M = 3.24, SD = 1.76) and (M = 2.74, SD = 1.63) statistically significant? Please report the result. If it was not tested, please state that as well.
5. On page 4, the sentence introducing Rayyan reads somewhat awkwardly for readers who may not be familiar with the platform. Consider briefly clarifying that Rayyan is a web-based software platform or screening tool used for systematic reviews before referring to it by name. This would improve readability and make the screening procedure easier to follow for readers outside the systematic review literature.
6. We found the distinction between "ACE characteristics in Islamist violent extremists" and "ACE characteristics in mixed populations" somewhat unclear. The latter section appears to include studies that sample individuals from multiple ideological backgrounds (e.g., right-wing, left-wing, and Islamist extremists), yet several findings are subsequently reported separately for specific ideological groups. As a result, it is not entirely clear whether "mixed populations" refers to studies that combine multiple extremist groups into a single sample, studies that compare different ideological groups, or studies examining extremists more broadly regardless of ideology. Consider clarifying the rationale for this categorization and potentially renaming the section to more accurately reflect its contents.
7. We occasionally struggled to keep track of the various populations included in the review. Throughout the manuscript, studies are described using terms such as Islamist extremists, jihadists, Salafi-jihadists, terrorists, suicide terrorists, armed group members, offenders, and violent extremists. While we appreciate that these labels originate from the primary studies, it is not always clear how the different categories relate to one another or whether they refer to ideological, behavioral, organizational, or legal classifications. Given that this is a review paper, and reiterating a comment from the previous round which was insufficiently addressed, we ask you to include a brief paragraph, either in the introduction or methods

section, clarifying how these different populations are conceptualized/defined within the review.

8. For consistency, please use either “violent extremism” or the abbreviation “VE” (after first defining it) throughout. Currently, after introducing the abbreviation, the manuscript later reverts to using the full term “violent extremism” on page 16. Consistently using one format would improve readability and avoid unnecessary switching between the abbreviation and the full term.

Given the minor nature of these revisions, we would like to receive your revised manuscript within three weeks. If you need additional time, please do not hesitate to reach out to us.

Best,
Jonas R. Kunst & Milan Obaidi

Reviewer 1

I thank the authors for their revisions. Overall, I believe that the manuscript has improved, and some of my comments have been addressed. However, I still see a few unresolved methodological issues.

1. The upper limit of the period covered by the literature search should be the exact date (not only ‘April’). I understand that the literature search has now been redone only in Scopus, and the remaining databases were not searched again. This should be reported, and exact search dates are needed for each database. Otherwise, the search is not replicable. Moreover, I still don’t find any explanation of how the ‘citation search’ was conducted (which papers’ reference lists were scanned? Why those?) and what expert advice means (how were those experts identified? Who were they, colleagues from the same institution, experts identified at a conference, just key experts active in the field?)

4. Thank you for reporting the agreement for abstract and full text screening. In my opinion, data extraction should also be conducted, at least in part (e.g., 20%), by two independent coders. Arguably, however, this requirement is less strict for reviews than meta-analyses, therefore I leave the decisions to the Editor here.

5. Thank you for extending the description of MMAT. I think the evaluation of study quality is a strength of this study, as not many scoping reviews use it. Even so, in my view the description remains insufficient. For instance, you state that it includes “design-specific criteria evaluating key aspects of methodological rigor including sampling strategy, risk of bias, appropriateness of statistical analysis…” In fact, all of these refer to the risk of bias. I would much rather see the full list of criteria, and the exact phrasing of the evaluation items you used. At the very least, some of these could be cited verbatim in the paper.

Please note that I don’t have access to the Appendices and I’m unable to check them.

Reviewer 2

Dear Editor,

Thank you for giving me the opportunity to review the revised version of the manuscript. I have read the revised text with interest, and I believe the manuscript now meets the standards for a scoping review on this topic. The authors have strengthened the rationale for why ACEs might matter for pathways into violent extremism, improved the criteria for primary study selection, and also strengthened the search strategy and the study quality appraisal. I also feel that the Discussion/Conclusion is now better balanced and that causal language is avoided. As such, I feel that the conceptual, methodological, and interpretive concerns that were raised have been substantively addressed by the authors. Two (very) minor suggestions for improvement:

(1). In the ACEs characteristics section (starting from line 199), the authors have flagged “[...], or parents who were never married” (line 205) as a primary-study-author-defined indicator of family disruption. Perhaps the authors could add a short note to the limitations paragraph of the Discussion stating that some ACE indicators (e.g., single-parent families and never-married parents) are culturally and contextually variable and may not always be seen as "family dysfunction", which affects cross-study comparability of results.

(2). The official name of the PsycInfo database is "APA PsycInfo" (see here: https://www.apa.org/pubs/databases/psycinfo).

Round 3

Editor

Date: 2026-07-15

Dear Authors,

Thanks a lot for addressing our remaining comments. We are glad to accept your review for publication in its present form. We believe it will be widely read and become an important contribution to the field! We also would like to thank the two reviewers for their thorough assessment and constructive reviews.

Sincerely,

Jonas R. Kunst & Milan Obaidi